The tides round the island of Skye are comparatively simple. The V. to XI. tide from the outside of Mull is gradually retarded to a VI. to XII. stream at the outside of Skye, and then as it rounds the north end of Skye, it is met by the tidal stream which has rounded the north end of the island of Lewis, and bends round into the inner Sound of Skye, where it becomes a VII. to I. tide; the course of both streams being nearly the same as if there were an embankment from Loch Shell in the island of Lewis to Ru Rea on the coast of Ross-shire. At the same time, another branch of the tide which has rounded the point of Ardnamurchan flows through the Sound of Skye as a XII. to VI. tide, and being an hour earlier than the tide which has rounded the north end of Skye, it pours with great velocity through Kyle Rea, but only to fill Loch Alsh and Loch Duich; the retardation which it meets with in so doing, making the rise inside of the narrows at Kyle Akin so nearly contemporaneous with the rise outside, that there is little stream through that narrow opening; the flood stream, as I am informed, sometimes flowing in one direction and sometimes in the other, according to the prevailing winds.

There are many more minute details in these streams which have features of great interest. I have not, however, ventured in the present imperfect state of the data which we possess to enter upon these. I venture, however, to express a hope that before the survey is completed, the data may be obtained for showing, and that the charts may show the direction and rate of the stream at every place and at any time.

March 15, 1866.

Lieut.-General SABINE, President, in the Chair.

The following communication was read:—

"On a possible Geological Cause of Changes in the Position of the Axis of the Earth's Crust." By JOHN EVANS, F.R.S., Sec. G.S. Received February 28, 1866.

At a time when the causes which have led to climatal changes in various parts of the globe are the subject of so much discussion, but little apology is needed for calling the attention of this Society to what possibly may have been one of these causes, though it has apparently hitherto escaped observation.

That great changes of climate have taken place, at all events in the northern hemisphere of the globe, is one of the best established facts of geology, and that corresponding changes have not been noticed to the same extent in the southern hemisphere may possibly be considered as due, rather to a more limited amount of geological observation, than to an absence of the phenomena indicative of such alterations in climatal conditions having occurred.

The evidence of the extreme refrigeration of this portion of the earth at the Glacial Period is constantly receiving fresh corroboration, and various theories have been proposed which account for this accession of cold in a more or less satisfactory manner.

Variations in the distribution of land and water, changes in the direction of the Gulf-stream, the greater or less eccentricity of the earth's orbit, the passage of the Solar System through a cold region in space, fluctuations in the amount of heat radiated by the sun, alternations of heat and cold in the northern and southern hemispheres, as consequent upon the precession of the equinoxes, and even changes in the position of the centre of gravity of the earth and consequent displacements of the polar axis, have all been adduced as causes calculated to produce the effects observed; and the reasoning founded on each of these data is no doubt familiar to all.

The possibility of any material change in the axis of rotation of the earth has been so distinctly denied by Laplace* and all succeeding astronomers, that any theory involving such a change, however tempting as affording a solution of certain difficulties, has been rejected by nearly all geologists as untenable.

Sir Henry James†, however, writing to the 'Athenæum' newspaper in 1860, stated that he had long since arrived at the conclusion that there was no possible explanation of some of the geological phenomena testifying to the climate at certain spots having greatly varied at different periods, without the supposition of constant changes in the position of the axis of the earth's rotation. He then, assuming as an admitted fact that the earth is at present a fluid mass with a hardened crust, showed that slaty cleavage, dislocations, and undulations in the various strata are results which might be expected from the crust of the earth having to assume a new external form, if caused to revolve on a new axis, and advanced the theory that the elevation of mountain-chains of larger extent than at present known produced these changes in the position of the poles.

The subject was discussed in further letters from Sir Henry James, the Astronomer Royal, Professors Beete Jukes and Hennessy, and others, but throughout the discussion the principal question at issue seems to have been whether any elevation of a mountain-mass could sensibly affect the position of the axis of rotation of the globe as a whole, and the general verdict was in the negative.

At an earlier period (1848) the late Sir John Lubbock, in a short but conclusive paper in the 'Quarterly Journal of the Geological Society'‡ pointed out what would have been the effect had the axis of rotation of the earth not originally corresponded with the axis of figure, and also mentioned some considerations which appear to have been absent from Laplace's calculations.

* Méc. Cél., vol. v. p. 14.

† Athenæum, Aug. 25, 1860, &c.

‡ Vol. v. p. 5.

Sir John Lubbock, however, in common with other astronomers, appears to have regarded the earth as consisting of a solid nucleus with a body of water distributed over a portion of its surface; and there can be but little doubt that, on this assumption of the solidity of the earth, the usually received doctrines as to the general persistence of the direction of the poles are almost unassailable.

Directly, however, that we argue from the contrary assumption that the solid portion of the globe consists of a comparatively thin, but to some extent rigid crust with a fluid nucleus of incandescent mineral matter within, and that this crust, from various causes, is liable to changes disturbing its equilibrium, it becomes apparent that such disturbances may lead, if not to a change in the position of the general axis of the globe, yet at all events to a change in the relative positions of the solid crust and the fluid nucleus, and in consequence to a change in the axis of rotation, so far as the former is concerned.

The existence in the centre of the globe of a mass of matter fluid by heat, though accepted as a fact by many, if not most geologists, has no doubt been called in question by some, and among them a few of great eminence. The gradual increase of temperature, however, which is found to take place as we descend beneath the surface of the earth, and which has been observed in mines and deep borings all over the world, the existence of hot springs, some of the temperature of boiling water, and the traces of volcanic action, either extinct or still in operation, which occur in all parts of the globe, afford strong arguments in favour of the hypothesis of central heat.

And though we are at present unacquainted with the exact law of the increment of heat at different depths, and though, no doubt, under enormous pressure the temperature of the fusing-point of all substances may be considerably raised, yet the fact of the heat increasing with the depth from the surface seems so well established that it is highly probable that at a certain depth such a degree of heat must be attained as would reduce all mineral matter with which we are acquainted into a state of fusion. When once this point was attained, it seems probable that there would be no very great variation in the temperature of the internal mass; but whether the whole is in one uniform state of fluidity, or whether there is a mass of solid matter in the centre of the fluid nucleus, are questions which do not affect the hypothesis about to be considered.

Those who are inclined to regard the earth as a solid or nearly solid mass throughout, consider that many volcanic phenomena may be accounted for on the chemical theory, which has received the support, among others, of Sir Charles Lyell. But apart from the consideration that such chemical action must of necessity be limited in its duration, the existence of local seas of fluid matter, resulting from the heat generated by intense chemical action, would hardly account for the increase of heat at great depths in places remote from volcanic centres; and the rapid transmission of shocks

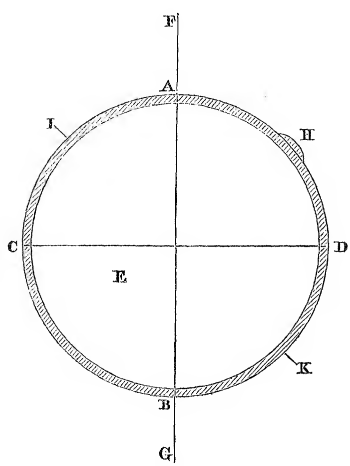
of earthquakes and the enormous amount of upheaval and subsidence as evidenced by the thickness of the sedimentary strata, seem inconsistent either with the general solidity of the globe or any very great thickness of its crust.

The supposition that the gradual oscillations of the surface of the earth, of which we have evidence all over the world as having taken place ever since the formation of the earliest known strata up to the present time, are due to the alternate inflation by gas and the subsequent depletion of certain vast bladdery cavities in the crust of the earth, can hardly be generally accepted.

Those who wish to see the arguments for and against the theory of there being a fluid nucleus within the earth's crust, will find them well and fairly stated in Naumann's 'Lehrbuch der Geognosie'*. My object is, not to discuss that question, but to point out what, assuming the theory to be true, would be some of the effects resulting from such a condition of things, more especially as affecting climatal changes. The agreement or disagreement between these hypothetical results and observed facts may ultimately assist in testing the truth of the assumption.

The simplest form in which we can conceive of the relations to each other of a solid crust and a fluid nucleus in rotation together is that of a sphere.

Let A C B D be a hollow sphere composed of solid materials and of perfectly uniform thickness and density, and let it be filled with the fluid matter E, over which the solid shell can freely move, and let the whole be in uniform rotation about an axis F G, the line C D representing the equator.



It is evident that in such a case, the hollow sphere being in perfect equilibrium, its axis and that of its fluid contents would perpetually coincide.

* 2nd edit., 1858, vol. i. p. 36.

If, however, the equilibrium of the shell or crust be destroyed, as, for instance, by the addition of a mass of extraneous matter at H, midway between the pole and the equator, not only would the position of the axis of rotation be slightly affected by the alteration in the position of the centre of gravity of the now irregular sphere, but the centrifugal force of the excess of matter at H would gradually draw over the shell towards D until, by sliding over the nucleus, it attained its greatest possible distance from the centre of revolution by arriving at the equator. The resultant effect would be that though the whole sphere continued to revolve around an axis as nearly as possible in the line F G, yet the position of the pole of the hollow shell would have been changed by 45°, as by the passage of H to the equator the points I and K would have been brought to the poles by spirals constantly decreasing in diameter, while A and B, by spirals constantly increasing, would have at last come to describe circles midway between the poles and the equator.

The axis of rotation of the hollow sphere and that of its fluid contents would now again coincide, and would continue to do so perpetually unless some fresh disturbance in the equilibrium of the shell took place.

If instead of the addition of fresh matter at H we had supposed an excavation or removal of some portion of the shell, a movement in the axis of rotation of the shell would also have ensued, since from the diminished centrifugal force of that portion of the hollow sphere where the excavation had taken place, it would no longer equipoise the corresponding portion on the opposite side at I, and the excavated spot would eventually find its way to the pole.

In order more clearly to exhibit these effects, I have prepared a model in accordance with a suggestion of Mr. Francis Galton, F.R.S., in which a wheel representing a section of a hollow sphere has its axis, upon which it can freely turn, fixed in a frame, which is itself made to revolve in such a manner that the axis of its rotation passes through one of the diameters of the wheel, and coincides with what would be the axis of the sphere of which the wheel is a section.

In the periphery of the wheel are a number of adjustable screws with heavy heads, so that, by screwing any of them in or out, the addition of matter or its abstraction at any part of the sphere may be represented.

If by adjusting these screws the wheel could be brought into perfect equilibrium, its position upon its own axis would remain unchanged in whatever position it was originally placed, notwithstanding any amount of rotation being given to the frame in which it is hung; but practically it is found that with a certain given position of the screws a certain part of the wheel coincides with the axis of the frame, or becomes the pole around which the sphere revolves. The rim of the wheel is graduated so as to show the position of the poles in all cases, and generally speaking the wheel always settles down after rotation with the pole within three or four degrees of the same spot, if no alteration has been made in the adjustment of the

screws, though of course what was the uppermost pole may become the lower one; and in some cases the wheel may be *in equilibrio* with a projecting screw either above or below the equator, in which case there may be four readings on the circle at the index-point, according as the one pole or the other is uppermost, and the projecting screw is above or below the equator.

With the screws on the wheel evenly balanced, a slight alteration in the adjustment of any of them immediately tells upon the position of what, for convenience sake, may be called the poles, except, indeed, in such cases as screwing outwards those already at the equator, or making similar alterations in the adjustment of two screws at equal distances on either side of one of the poles. If a screw be turned outwards so as notably to project at any spot, no matter how near to the pole, it will be found, after the machine has been a short time in revolution, in the region of the equator. Or again, if one or, better still, two opposite screws at the equator be turned inwards, they will be found after a short period of revolution at the poles.

Now let us assume for a moment that, though the crust was partially covered by water, the earth, instead of being a spheroid, was a perfect sphere, consisting of a hardened crust of moderate thickness supported on a fluid nucleus over which the crust could travel freely in any direction, but both impressed with the same original rotatory motion, so that without some disturbing cause they would continue to revolve for ever upon the same axis, and as if they were one homogeneous body. Let us assume, moreover, that this crust, though in perfect equilibrium on its centre of rotation, was not evenly spherical externally, but had certain projecting portions, such as would be represented in Nature by continents and islands rising above the level of the sea.

It is evident that so long as those continents and islands remained unaltered in their condition and extent, the relative position of the crust to the enclosed fluid nucleus would remain unaltered also. But supposing those projecting masses were either further upheaved from some internal cause, or worn down and ground away by the sea or by subaërial agency and deposited elsewhere, it seems impossible but that the same effects must ensue as we see resulting upon the model from the elevation and depression of certain screws, and that the axis of rotation of the crust of the sphere would be changed in consequence of its having assumed a fresh position upon its fluid nucleus, though the axis of the whole sphere might have retained its original direction, or have altered from it only in the slightest degree.

An irregular accumulation of ice at one or both of the poles, such as supposed by M. Adhémar, would act in the same manner as an elevation of the land; and even assuming that the whole land had disappeared from above the surface of the sea, yet if by marine currents the shallower parts of the universal ocean were deepened and the deeper parts filled up,

there would, owing to the different specific gravity of the transported soil and the displaced water, be a disturbance in the equilibrium of the crust, and a consequent change in the position of its axis of rotation.

Now if all this be true of a sphere, it will also, subject to certain modifications, be true of a spheroid so slightly oblate as our globe.

The main difference in the two cases is, that in a sphere the crust may assume any position upon the nucleus without any alteration in its structure, while in the case of the movement of a spheroidal crust over a similar spheroidal nucleus, every portion of its internal structure must be more or less disturbed as the curvature at each point will be slightly altered.

The extent of the resistance to an alteration of position arising from this cause will depend upon the oblateness of the spheroid and the thickness and rigidity of the crust; while the thicker the latter is, the less also will be the proportionate effect of such elevations, subsidences, and denudations as those with which we are acquainted. The question of friction upon the nucleus is also one that would have to be considered, as the internal matter though fluid might be viscous.

It will of course be borne in mind that the elevations and depressions of the surface of the globe are not, on the theory now under consideration, regarded according to the proportion they bear to the earth's radius, but according to their relation to the thickness of the earth's crust; and that, even assuming Mr. Hopkins's extreme estimate to be true, yet elevations or depressions, such as we know to have taken place, of 8000 or 10,000 feet, bear an appreciable ratio to the 800 or 1000 miles which he assigns as the thickness of the earth's crust.

It is, however, to be remarked that the extremely ingenious speculations of Mr. Hopkins are based on the phenomena of precession and nutation, and that if once the possibility of a change in the position of the axis of rotation of the earth's crust be admitted, it is not improbable that the value of some of the data upon which the calculations of these movements are founded may be affected.

The supposition of the thickness of the crust being so great seems also not only entirely at variance with observed facts as to the increase of heat on descending beneath the surface of the earth, but to have been felt by Mr. Hopkins himself to offer such obstacles to any communication between the surface of the globe and its interior, that he has had recourse to an hypothesis of large spaces in the crust at no great depth from the surface, and filled with easily-fusible materials, in order to account for volcanic and other phenomena.

But though it may be possible to account for volcanoes upon such an assumption, yet, as already observed, the phenomena of elevation and depression, such as we find to have taken place, and more especially the existence of vast geological faults, cannot without enormous difficulty be reconciled with such a theory.

Taking the increment of heat as 1° Fahrenheit for every 55 or 60 feet* in descent, a temperature of 2400° Fahr. would be reached at about 25 miles, sufficient to keep in fusion such rocks as basalt, greenstone, and porphyry; and such a thickness appears much more consistent with the fluctuations in level, and the internal contortions and fractures of the crust which are everywhere to be observed. Sir William Armstrong, on the assumption of the temperature of subterranean fusion being 3000° Fahr., considers that the thickness of the film which separates us from the fiery ocean beneath would be about 34 miles.

Even assuming a thickness of 50 miles, so as to make still greater allowance for the increased difficulty of fusion under heavy pressure, the thickness of the crust would only form one-eightieth part of the radius of the earth; or if we represent the earth by a globe 13 feet in diameter, the crust would be one inch in thickness, while the difference between the polar and equatorial diameters would be half an inch.

In such a case, the elevation or wearing away of continents such as are at present in existence, rising, as some of them do, nearly a quarter of a mile on an average above the mean sea-level, would cause a great disturbance in the equilibrium of the crust, sufficient to overcome considerable resistance in its attempts to regain a state of equilibrium by a movement over its fluid nucleus.

Whether the thickness of the earth's crust was not in early geological times less than at present, so as to render it more susceptible of alterations in position—whether the spheroid of the fluid mineral nucleus corresponds in form with the spheroid of water which gives the general contour of the globe—whether or no there are elevations and depressions upon the nucleus corresponding to some extent with the configuration of the outer crust, and whether the motion of the crust upon it, besides effecting climatal changes, might not also lead to some elevations and depressions of the land, and produce some of the other phenomena mentioned by Sir Henry James, are questions which I will leave for others to discuss.

My object is simply to call attention to what appears to me the fact, that if, as there seems reason to suppose, our globe consists of a solid crust of no great thickness resting on a fluid nucleus, either with or without a solid central core, and if this crust, as there is abundant evidence to prove, is liable to great disturbances in its equilibrium, then it of necessity follows that changes take place in the position of the crust with regard to the nucleus, and an alteration in the position of the axis of rotation, so far as the surface of the earth is concerned, ensues.

Without in the slightest degree undervaluing other causes which may lead to climatal changes, I think that possibly we may have here a *vera causa* such as would account for extreme variations from a Tropical to an Arctic temperature at the same spot, in a simpler and more satisfactory manner than any other hypothesis.

* Page, 'Advanced Text-book of Geology,' p. 30.

The former existence of cold in what are now warm latitudes might, and probably did in part, arise from other causes than a change in the axis of rotation, but no other hypothesis can well account for the existence of traces of an almost tropical vegetation within the Arctic circle.

Of the former existence of such a vegetation, the evidence, though strong, is not conclusive. But if the fossil plants of Melville Island, in lat. 75° N.*, which appear to agree generically with those from the English coal-measures, really grew upon the spot where they were now discovered, they seem to afford conclusive evidence of a change in the position of the pole since the period at which they grew, as such vegetation must be considered impossible in so high a latitude.

The corals and Orthoceratites from Griffiths Island and Cornwallis Island, and the liassic Ammonites from Point Wilkie, Prince Patrick's Island, tell the same story of the former existence of something like a subtropical climate at places at present well within the Arctic circle.

To use the words of the Rev. Samuel Haughton†, in describing the fossils collected by Sir F. L. McClintock, "The discovery of such fossils *in situ*, in 76° N. latitude, is calculated to throw considerable doubt upon the theories of climate, which would account for all past changes of temperature by changes in the relative position of land and water on the earth's surface;" and I think that all geologists will agree with this remark, and feel that if the possibility of a change in the position of the axis of rotation of the crust of the earth were once admitted, it would smooth over many difficulties they now encounter.

That some such change is indeed taking place at the present moment may not unreasonably be inferred from the observations of the Astronomer Royal, who, in his Report to the Board of Visitors for 1861, makes use of the following language, though "only for the sake of embodying his description of the observed facts," as he refers the discrepancies noticed to "some peculiarity of the instrument. . . . . The Transit Circle and Collimators still present those appearances of agreement between themselves and of change with respect to the stars which seem explicable only on one of two suppositions—that the ground itself shifts with respect to the general Earth, or that the Axis of Rotation changes its position."

March 22, 1866.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

* Lyell, 'Principles of Geology,' 1853, p. 88.

† Journal of the Royal Dublin Society, vol. i. p. 244.